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REMARKS  
ON THE  
MEDICAL THEORIES  
OF  
BROWN, CULLEN, DARWIN, & RUSH.

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BY JOHN T. REES,

OF MARYLAND.....MEMBER OF THE AMERICAN LINNEAN AND  
HONORARY MEMBER OF THE PHILADELPHIA MEDICAL  
SOCIETIES.

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“ Think, read, and observe. Observe, read, and  
think for yourselves.”

RUSH.....*An Introductory Lecture.*

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1805.





AN INAUGURAL DISSERTATION  
FOR  
THE DEGREE  
OF  
DOCTOR OF MEDICINE;

SUBMITTED  
*TO THE EXAMINATION*  
OF THE  
MEDICAL PROFESSORS,  
THE  
REV. JOHN ANDREWS, D. D. PROVOST  
(PRO TEMPORE);  
AND THE TRUSTEES  
OF THE  
UNIVERSITY OF PENNSYLVANIA,

ON THE FIFTH DAY OF JUNE, 1805.

AN INDUCTIVE DISSERTATION

FOR

THE DEGREE

OF

DOCTOR OF MEDICINE

SUBMITTED

TO THE FACULTY

OF THE

MEDICAL PROFESSION

THE

REV. JOHN ANDREW, D. D. PROFESSOR

(OF THE FACULTY)

AND THE FACULTY

OF THE

UNIVERSITY OF PENNSYLVANIA

ON THE FIRST DAY OF JUNE, 1880.



TO  
DOCTOR JOHN THOMAS,  
OF THE HEAD OF CHESTER,  
MARYLAND.

DEAR SIR,

TO you I dedicate this essay....not as a formal compliment....but as a mark of the warmest gratitude.

Under your fostering care I passed my childhood; and your friendly counsels have directed the conduct of my youth. You pointed out the superiority of *intellectual* enjoyment; you urged me to the walks of science; and your example taught me, that her votaries are respectable and happy.

In prefixing, then, your name to these the *inaugural fruits* of my medical studies, I feel a particular pleasure; though that pleasure would be greater, were the essay more worthy of your attention and patronage.

That you may long enjoy the happiness of health, which you have so frequently, and so ably, restored to others, is the sincere wish of

Your Nephew,  
Friend,  
And former Pupil,  
THE AUTHOR.



TO  
BENJAMIN RUSH, M. D.

PROFESSOR OF THE INSTITUTES OF MEDICINE IN THE  
UNIVERSITY OF PENNSYLVANIA.

DEAR SIR,

THE motives that have induced me to prefix your name also to this essay need not be enumerated. Genuine politeness and pleasing manners, added to high medical talents, have always inspired your students with love and admiration. My bosom glows in reviewing the happy years I spent under your private tuition....Be assured, Sir, they will always be remembered by me, with gratitude and respect.

For the cause of virtue and of humanity, and for the progress of medical science, may your life be lengthened to a long....long old age:

Accept the high esteem of

Your Friend

And Pupil,

JOHN T. REES.



TO

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## PRELIMINARY.

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THAT every candidate for medical honors shall give to the public some specimen of his acquirements, is a duty imposed on him by the laws of the university; and with the view *solely* of discharging this duty, I submit the following essay. It will be found to contain remarks, or criticisms, on the theories of four medical characters of the first eminence in the profession; whether they be just or not, the reader must determine....I however contend for their *originality*.

To the candid and discerning, it will be a sufficient apology for imperfections in style, &c. to observe; that owing to *particular* circumstances, the author had but a short time (only a *part* of that, allotted to this purpose) to write the essay in; and little or none to correct it. To readers of any other description, no appeal is made.





## AN INAUGURAL THESIS, &c.

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MEDICAL Philosophers may be properly divided into two classes: Experimenters and Theorists; both of whom have carried their prejudices to extremes. It is no uncommon thing, to hear the experimenter boast much of the importance of his experiments; the fairness with which they were made, and the many hidden mysteries they so plainly lay open; at the same time loudly vociferating against the fallacy of theory: not recollecting that "to think is to theorize," and that "without thinking, that is, without theorizing," he could not draw a conclusion even from his own experiments. While, on the other hand, we see the theorist, not only relying exclusively on speculation, but also deserting those paths which should alone be followed, in all theoretical pursuits. Shamefully neglecting that grand maxim, of "proceeding only from things known to things unknown;" which should always be his rallying point, perpetually his polar star: we see him illiberally passing over facts that would militate against, twisting others from their natural meaning, imagining those that never existed; and, out of these slender, these brittle materials, erecting medical fabrics, that must inevitably fall be-

fore the first breath of reason.... Thus shamefully abusing the beautiful science of cause and effect.

But, although these objections may be brought against the abuse of theory, they certainly cannot be applied to theory, when it is conducted on rational and proper principles.

Since, then, "to think is to theorize," we must all *theorize* who *think* on medicine. As much the illiterate quack, who gives his pill to sweeten the blood, or to purify the urine; as the philosophic practitioner, who bleeds to relieve suffocated excitement, and thus resuscitate the strangulated blood vessels. The difference consists in their *manner* of theorizing. The one proceeds in that order, which has been laid down by the wisdom of former sages, and approved of by the experience of past ages; taking such positions only, as are known to be true, and drawing his inductions from preceding facts. While the other proceeds disorderly, without either method or science to assist him, and forms his opinions, either from popular reports, upon imaginary truths, from very weak reasoning, or, what is more frequently the case, without any *reason* at all.

Having made these remarks, we shall not pause further, to prove the utility of correct theory, when applied to the science of medicine; but shall just observe, that, in forming theories of our own, we should proceed with caution; and when those of others are offered us, we should scan them with a critical and a jealous eye. Thus, while correct principles are much to be wished



for on the one hand, so the hasty rejection of those that are false, or dangerous, is equally desirable on the other.....But, to return to the experimenters.

Have this set of medical philosophers banished error from our science; diffused light on all the subjects they have attempted to investigate; and placed medicine on as simple, and as sure a footing as the mathematics? No: very different indeed. They have opposed contradiction, with contradictions; refuted one error, with another; and multiplied doubts on many physiological points. Thus, for instance, the doctrine of absorption has been bandied about by hundreds; now fairly proved to take place, and now as fairly disproved ever to occur.

But we should not be surprised, to find experiments run counter, when we consider the variety of changes to which the system is liable. To expect the same results from an experiment, made on the body in high health, while every part performs with vigor its natural function; and the same experiment, made when the body is diseased and debilitated, would betray the grossest ignorance of the animal œconomy.

From the great number of experimenters, only a few, we believe, have benefited the science of physiology; and of these, the Abbé Spallanzani stands among the highest. Upon the subject of fecundation, he is truly meritorious. It was this great Italian who first raised the curtain of nature, and dared to look with boldness on her



scenery: nay, he did more; he qualified himself by industry and became an actor on her stage. But, for this one instance of worth and ingenuity in an experimenter, we have many, where no confidence should be placed in their opinions. Nor is Spallanzani entirely an exception. Although his experiments are decisive and invaluable, yet, the conclusions drawn from them are not so plausible. Thus, after having impregnated the ovum of the female, by touching it with the semen of the male frog, he concludes, that the tadpole pre-existed in the ovum; and that the semen only stimulated it into existence. But what are his arguments? Let him answer for himself; he could not *discover* a difference between the impregnated and unimpregnated ovum, by inspecting them with his *glass*; poor reasoning! a very ridiculous theory! Rash philosopher, bigotted experimenter, can your *powerful* glass distinguish between an atmosphere loaded with contagion, and one that is not contaminated? No, surely it can not; and yet, as surely, an important difference *does* exist.

But, it may be asked, if this be not the correct method of accounting for fecundation, what is the process? We believe it to be entirely chemical; the male semen uniting with the female ovum, and producing a third something, differing from them both in its properties. Thus, upon adding an acid to an alkali, they unite to form a tertium quid; the properties of which are also essentially different from those of the materials which compound it. The reason why we have a

neutral salt in the one instance, and a fœtus in the other is simply, because the matter of the two is different. This, we conceive, to be the whole phœnomenon of fecundation. To say that the semen stimulates the ovum, and thus vivifies the fœtus, is giving no explanation at all; we hardly know what it means: surely no one will be so absurd as to say, that the acid stimulates the alkali, and thus produces the neutral salt....But we wander from our subject; it is not the process of fecundation we proposed considering; but the medical theories of Brown, Cullen, Darwin, and Rush.

Pardon this digression. We now hasten with pleasure to the task; and shall begin with the

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## BRUNONIAN THEORY.

I. Since boldness of thought and originality of conception appear to be the rage, the grand criterion by which we are to judge of an author's merit, and almost the *ne plus ultra* of his desire, we shall inquire into the claim that doctor Brown has to originality: but, previous to our doing this, it will be proper to sketch the outlines of his theory.

1. That property in the animal system, which renders it alive to the impressions of stimuli, or by which actions are produced, when certain agents are applied, Brown calls the *excitability*; the agents he calls *exciting powers*, or *stimuli*; and the actions produced he calls *excitement*.



2. The excitability is supposed to be an unit; that is, when a stimulus is applied to one part of the system, it produces a certain effect in that part, and, by means of the unity in the excitability, also produces the same effect, more or less, in every other part.

3. The exciting powers, or stimuli, are supposed to be of great variety; but to differ from each other, only in their degree of stimulus.

4. The excitement, also, is supposed to vary only in degree.

5. When the excitability, exciting powers, and excitement, are at a medium, and equal in degree, the system is said to be in a state of perfect health: and when the excitement is increased much above or below what is natural, the system is said to be diseased.

6. All diseases are reduced to two kinds, called *Sthenic* and *Asthenic*. Sthenic when the excitement is increased. Asthenic when it is diminished.

7. When a portion of the exciting powers is abstracted, the excitability becomes accumulated. And when they are applied in an excessive quantity, an exhaustion of it follows.

8. When the excitability is accumulated, even a moderate stimulus will produce an increased excitement or sthenic disease: and when it is nearly exhausted, it requires a powerful stimulus to produce any effect.

9. When a deficient portion of stimulus is applied to the system, the excitement becomes di-



minished, and *direct* debility is induced. And when stimulus has been applied in an excessive quantity, the excitement also becomes diminished, and the system is then said to be in a state of *indirect* debility. Both of these debilities are clas-  
 sed together under the general term of asthenic disease ; but with this difference, that, in direct debility, the excitability is accumulated ; while in indirect, it is exhausted.

10. The cure of asthenic disease is to be effected by the application of stimuli, as opium, musk, volatile spirits, brandy, a nourishing diet, &c. While sthenic disease is to be cured by the abstraction of stimuli, as by blood-letting, purging, cool air, low diet, &c.

II. Having thus stated the grand fundamental principles of the Brunonian theory ; we now proceed to bring into view the principles of one or two ancient authors ; and leave the reader to determine, how far Dr. Brown's claim to originality may be just.

There appears to be something like periodical revolutions in the science of medicine. As far back as the days of Hippocrates, we find, the humoral pathology was the prevailing doctrine ; and the terms, *morbific matter*, *nature*, and *concoction*, were as familiarly used then ; as those of *excitability*, *stimulus*, and *excitement*, are at the present day. This doctrine was succeeded, about three hundred years afterwards, by that of Asclepiades, which rejected much of the humoral pathology.

This was again followed by one, proposed by Themison, the pupil of Asclepiades, which placed disease altogether in the solids. Some time after this, Galen revived the humoral pathology ; which continued, more or less, in every medical system, down to the times of Hoffman and Cullen. These medical worthies again introduced the pathology of the solids ; and the humoral pathology is now nearly exploded.

From the theories of Asclepiades and Themison, we think Brown may have profited something : at any rate, the features of his theory and the features of theirs have a strong family likeness.

“Asclepiades asserted, that matter, considered in itself, was of an unchangeable nature ; and that all perceptible bodies were composed of a number of smaller ones, between which there were interspersed an infinity of small spaces, totally devoid of all matter. He thought that the soul itself was composed of these small bodies. He laughed at the principle called *nature* by Hippocrates, and also at the imaginary faculties said by him to be subservient to her ; he also maintained, that nothing happened or was produced without some cause ; and that, what was called *nature*, was, in reality, no more than *matter* and *motion*. From this last principle he inferred, that Hippocrates knew not what he said when he spoke of nature as an intelligent being, and ascribed qualities of different kinds to her. According to him, Hippocrates and other physicians attended their patients rather with a view to observe in what manner they



died, than in order to cure them; and this under pretence that nature ought to do all herself, without any assistance." For an account of the principles of Asclepiades, the reader may consult the history of medicine, contained in the Encyclopædia.

But it is the theory of Themison that, we think, bears so considerable an analogy to Brown's.

"Themison divided all diseases into two, or at most into three kinds. The first included diseases arising from *stricture*; the second, those arising from *relaxation*; and the third, those of a mixed nature, or such as partook both of stricture and relaxation. He said, that all disorders, whatever their nature was, if included under any of the above-mentioned kinds, ought to be treated precisely in the same way, in whatever country, and with whatever symptoms, they happen to arise.

"Themison was old when he laid the foundation of this methodic sect; and it was only brought to perfection by Thessalus, who lived under the emperor Nero. Galen and Pliny accuse this physician of intolerable insolence and vanity; and report, that he gave himself the air of despising all other physicians; and so intolerable was his vanity, that he assumed the title of *Conqueror of Physicians*; which he caused to be put upon his tomb in the Appian way."

From these extracts the reader will perceive, that Asclepiades ridiculed the doctrine of nature's curing disease, some hundred years before Brown



did, and pretty much in the same way ; and that his opinions concerning *matter* and *motion* may probably have suggested the ideas of excitability and excitement.

And also, that Themison had long ago anticipated Brown in attempting to simplify medical science, by reducing all diseases to the two principal and opposite kinds of *stricture* and *relaxation*; for Brown has, eighteen hundred years after him, only done the same. The difference consists only in the different *terms* they use, to express the *same state* of the system.

Themison was sanguine in his principles ; so was Brown. Themison declares that the symptoms, climate, &c. that may be connected with a disease, cannot at all alter its mode of cure, provided we ascertain the class to which it belongs; so does Brown, in every part of his work.

Are Brown's disciples zealous and dogmatic? So were the followers of Themison; even so much so, that Thessalus had the presumption to style himself *Conqueror of Physicians*.

III. Having thus briefly shewn the analogy between the Brunonian theory and the combined theories of Asclepiades and Themison, we proceed to examine its merits.

In doing this, we shall not notice the theory of any particular disease; as for instance, of fever, which the doctor erroneously supposes to consist in debility; and we shall also pass over many other points, on which criticisms might be offered. We shall attack only a few fundamental

principles; thus striking, as it were, at the pillars of his doctrine; knowing, that if these give way, the whole fabric must tumble.

1. Dr. Brown supposes, as will be seen by consulting his theory, that there is a kind of unity in the excitability, by means of which, when one kind of excitement, either increased or decreased, takes place in one part of the system, it will also take place, more or less, in every other part.

Now, according to this position, two diseases cannot exist in the system at the same time.

For, as disease consists only in different degrees of the same excitement; as no two different degrees of excitement can exist in the same part at the same time; and, as the excitement existing in one part must, by the unity of the excitability exist, more or less, in every other part; it follows, that fever which is a disease of the blood vessels, and the venereal disease, which is seated in the glands, cannot exist together in the system. Because the venereal action in the glands must be extended to the blood vessels also, and thus prevent the fever.

But, were it possible for a fever to occur, then, at that point where the venereal action ceased, and the febrile one commenced, there would be a kind of *tertium quid*; which, we believe, has never been discovered.

Again, we often see high excitement in the blood vessels, during a paroxysm of fever, while the muscles are preternaturally weak. And, on the other hand, the excitement of the muscles in tetanus and other convulsive affections, is highly



increased, while the action of the blood vessels remains little, or not at all, affected. These facts, we take it, are strongly opposed to unity in the excitability; for, according to this, the muscles ought during a fever to be preternaturally strong, instead of being preternaturally weak; and *vice versa*, the blood vessels should partake of the increased action of the muscles, in convulsive diseases.

2. The Brunonian theory asserts, that there is no other difference between the various stimuli that act on the system, than what exists in their degree of force or stimulating power.

According to this doctrine, if a sufficient quantity of brandy be taken, which is a strong stimulus, it ought to support life without the aid of food; but this we know, is quite the contrary.... Again the idea of one stimulus differing from another, only in degree of force, is entirely set aside by one or two very familiar facts. Thus, cantharides when taken into the stomach, act by their stimulus on the urinary organs, and frequently inflame the bladder; while tartar emetic, also taken into the stomach, acts particularly on this organ, and inverts its motion; the stimulus of julep purges by acting on the intestines; the stimulus of opium acts upon the system at large, producing hilarity, &c. ; that of sound acts upon the ears, of light upon the eyes, and of heat upon the general surface; but it is the stimulus of *food* only, that nourishes and supports the body. From these facts, it is clear, that stimuli have been, by others,



properly divided into specific; or in other words, that different stimuli are peculiarly adapted to act on different parts of the system.

3. This theory supposes disease to consist only of a variation in the degree of common or healthy excitement.

If this be all that constitutes disease, it is obvious, that a person passing through all the different degrees of excitement, during the exercises of the day, until his system, at night, arrives at the point of indirect debility, when sleep comes on, must, before this state can be induced, pass through every grade of *sthenic disease*; as through the rheumatism, peripneumonia, catarrh, &c.&c.; and that, during this state of sleep, he must be labouring under cholic, cholera, diabetes, or some other of the asthenic diseases. This must be the conclusion, because rheumatism, catarrh, peripneumonia, and all other sthenic diseases, are supposed to consist in an increased excitement only, which must exist somewhere on the scale, between the points of indirect debility and perfect health; of course, the system, in its progress to indirect debility, having to pass through all the intermediate degrees between those two points, must also pass *through* all the sthenic diseases, before that state can be induced, and consequently before sleep can take place. In fine, we should labour under sthenic disease every day, and under asthenic every night. These, however, are absurdities so great, that no one would advance them; and yet these are really the inductions to be drawn from the above principles.

4. It is contended, in the theory we are considering, that indirect debility is always attended with an exhaustion of excitability; and that it can be cured, only by stimulating remedies.

Whether the first part of this position be correct or not, may probably remain doubtful, though the following fact would seem to militate against it: thus if a person fatigue himself suddenly, as by riding fast on horseback when not accustomed to it, or by any other fatiguing exercise, and a glass or so of wine be now taken, it will produce twice the effect the same quantity would have done before, and will frequently intoxicate; which we think can only be accounted for, by supposing, that, in this instance, the fatigue or indirect debility is accompanied with an increase of excitability

The latter part of the above position, however, we are sure, is incorrect; and common experience proves it so. For instance, the indirect debility that is brought on by exercise during the day, as by walking, riding, labouring, &c. is removed or cured during the night, not by the *application* of stimuli, but by their *abstraction*; as by rest, a recumbent posture, silence, darkness, &c. &c.

Having thus shewn, that indirect debility can be cured, and frequently is, by the abstraction of stimuli, let us examine the propriety of attempting to cure it on the stimulating plan. For instance, a stimulus has brought on indirect debility, and, according to Brown, an exhaustion of excitability; yet, another stimulus of a different



kind, we are told, will find a small portion of excitability remaining, and by acting on it will produce excitement; and thus by degrees remove the indirect debility. Admitting a new stimulus may find a small portion of excitability remaining, and produce excitement by acting on it, still nothing is gained; for this stimulus must in its turn exhaust that remaining portion; and of course, instead of removing the debility, sink the system lower than it was before.

5. Lastly, *strict* reasoning on the Brunonian principles will lead to an egregious error; it will prove, that direct debility can never occur.

Thus, it is received as an established law of the excitability, stimulus, and excitement, that the excitability becomes accumulated in proportion to the abstraction of stimuli; and also, that stimuli act and produce excitement, in proportion to the accumulation of excitability. Now, reasoning from these principles, it will appear, that direct debility can never take place: for instance, suppose that all the different stimuli that act upon the system are as  $40^{\circ}$ ; and, that, acting upon the excitability at  $30^{\circ}$ , they produce healthy excitement at  $20^{\circ}$ ; it is obvious, if you abstract 10 degrees of stimuli, you accumulate the excitability  $10^{\circ}$ , and the remaining 30 degrees of stimuli, acting upon the excitability now accumulated to  $40^{\circ}$ , will still produce healthy excitement at  $20^{\circ}$ ; for stimulus acts in proportion to the accumulation of excitability. So on, extend the reasoning and principles as far as you please; still they will lead to the above conclusions. They



will also prove, that neither increased action nor indirect debility can ever be induced; and in fine, that the excitement, and excitability must remain stationary; all of which, however, we know is incorrect.

Thus then we see, that the Brunonian theory like a rocket ascending on a dark evening, was calculated to draw attention, only for the moment, pleasing by its splendour; to afterwards explode and disappear for ever.

Although we have been so unreserved in objecting to this theory of Dr. Brown's, let it not be presumed we are insensible of its author's merit, or would wish for a moment, to treat him with the least disrespect. We admire and revere his talents; and trust we have handled his opinions with that freedom only, to which the works of great men *should* be subjected. We view Brown as the first, among the moderns, who began to clear away the rubbish from our science: as a pioneer in medicine, who has cut the road for thousands: and we reflect with regret on his misfortunes. If, therefore, we have trodden roughly on his ashes, it was not intended. Honour to his genius!...Fame to such of his opinions as are true; and oblivion to the errors of his head and heart.



## CULLENIAN THEORY.

I. It would be a difficult and tedious task, to follow Dr. Cullen through all his theories, or

rather his conjectures, of different diseases; and our time will not permit it. We shall therefore notice, only his theory of fever; for this is his principal, and almost his only one; that has attracted the attention of physicians.

The reader is referred to the author's First Lines for a full detail of his theory: we shall, however, sketch its general features.

1. Certain debilitating powers applied to the system, diminish the energy of the brain, and thus bring on torpor, or debility, which soon produces the cold fit.

2. When this occurs, a certain power in the system, called the *vis medicatrix naturæ*, is excited into action, in order to remove this cold state and debility; and which, to effect this, induces a spasm on the extremities of the arteries, particularly of those that terminate on the surface of the body.

3. This spasm acts as an irritant to the heart and arteries, and increases their action, which constitutes the hot fit of fever: this removing the debility and cold stage immediately on its taking place, and finally the spasm itself; the system returns to its former state of health.

To give a summary in the author's own words, "Upon the whole, our doctrine of fever is explicitly this: the remote causes are certain sedative powers, applied to the nervous system, which diminishing the energy of the brain, thereby produce a debility in the whole of the functions, and particularly in the action of the extreme vessels. Such, however, is, at the same time, the nature



of the animal œconomy, that this debility proves an indirect stimulus to the sanguiferous system; whence, by the intervention of the cold stage and spasm connected with it, the action of the heart and arteries is increased, and continues so until it has had the effect of restoring the energy of the brain, of extending this energy to the extreme vessels, of restoring therefore their action, and thereby especially overcoming the spasm affecting them; upon the removal of which, the excretion of sweat, and other marks of the relaxation of excretories, take place." Cullen's First Lines, Paragraph xlvi.

II. This theory was not original with Cullen; he took it from Hoffman; Hoffman from Van Helmont; and probably Van Helmont may have taken it from some other. However, it is certain that Cullen has improved the original doctrine, filtered it from the humoral pathology, added many facts, and extended the principles. The *vis medicatrix naturæ*, so long and so well known in the schools of medicine, constitutes the basis of this theory. But, in whatever shape we admit this *hobby*, whether as the *autocrateia* of Hippocrates, the *archeus* of Van Helmont, the *anima medica* of Sthaal, or the *vis medicatrix naturæ* of Hoffman and of Cullen; still if intelligence be granted it, we must expect to meet with feeble and vague theory. To account for any phenomenon by referring it to the agency of this power, is not accounting for it at all. It would be just



as good reasoning to say a thing is so, *because* it is so.

If, however, the term *vis medicatrix naturæ*, have been used to express only a peculiar property in the system, we do not see its great difference from our modern excitability: and from this circumstance, that Cullen, as well as Hoffman, criticised on the opinions of Sthaal, who granted an intelligence to his *anima medica*, we should suppose that he used the expression in this sense only. But this was not the case: for however absurd the doctrine of Sthaal may have appeared to Cullen, yet, when difficulties arise, and explanations are scarce, we always find him to take shelter under this *sanctuary*; which, like the horns of an altar, protect him from the adversaries of his theory.

III. From the nature then of Dr. Cullen's theory it can admit of but few remarks; we shall therefore not refrain any longer, but overset it at once, by *denying* him his *vis medicatrix*. But as there are some powerful facts which stand forth in defence of this doctrine, we think it our duty to state them. Thus, it has been observed by John Hunter, that when the broken ends of a fractured bone are placed in apposition, they throw out osseous matter, into which the old vessels shoot; and thus the bone becomes reunited; this is common and is not surprising. But he has also observed that if the broken ends, instead of being placed nicely together, lap over

each other so as to bring their smooth surfaces into contact ; osseous matter is thrown out from these smooth sides, into which vessels shoot ; and the bone, as in the former instance, becomes reunited. It is easy, we say, to conceive how the vessels of the broken ends of a bone, may throw out osseous matter, and thus unite the bone ; but how, or from what cause, the vessels of one smooth surface, which is in contact with another, should throw out bony matter, and produce an union between them ; can not be easily accounted for.

Again, Astley Cooper has observed, that when a divided intestine is united with the needle, and returned into the abdomen and the wound heals, the ligature, instead of sloughing off, from the peritoneal side of the intestine, and thus lodging in the cavity of the abdomen, where it would prove injurious ; is universally thrown off into the cavity of the intestine, and thence discharged with its other contents.

Although these, with some other facts that could be mentioned, would argue something like intelligence in the operations of the system ; still we are not disposed to adopt this doctrine. For though we can not now explain these facts, on the laws of physical necessity, yet this may be owing to our imperfect acquaintance with those laws ; and hereafter, when our views of them become more enlarged, we may be able to explain, not only these, but many other difficulties. But if we admit this doctrine of intelligence in the opera-



tions of the system ; what shall we gain ? Surely nothing : on the contrary it will check the thirst of inquiry, and lead to the erroneous conclusions, that nature alone is sufficient to cure disease, and that medical assistance is either hurtful or superfluous.

This doctrine, however, should not be admitted for a number of reasons ; but particularly, because we have others, that are much more satisfactory. Thus, the animal system is capable of being affected, when exposed to the action of certain agents : and this capability depends on certain properties or qualities, natural to the system, and coeval with its formation, and which is also regulated by certain laws.

Now, by this one clue, we are enabled to penetrate the labyrinth of nature, and plant directing posts at every corner.

For instance, when certain debilitating powers have been applied to the body, its actions become diminished, generally ; but particularly in the heart and arteries : hence these propel the blood, with less force, to their extremities, which, in consequence, fall into a collapsed, not a spasmodic, state ; hence we account for the paleness, shrinking, and coldness of the skin, and, in short, for all those symptoms which constitute the cold stage of fever. But the capability for action, or in other words, the excitability becomes accumulated in consequence of the debility, for this is one of its laws. Of course the common stimuli or agents, as the aliment, blood, secretions, heat, air, &c. acting



on an increased capacity, must produce an increased action, which will be the greatest in those parts possessing the greatest capacity: but this capacity is the greatest in the heart and arteries, in consequence of the great debility, with which they were affected during the cold stage; hence their action becomes increased, the blood is driven with vigor to the surface, an increase of secretions takes place, which are attended with an increase of heat, the collapsed state disappears, flushings come on; and hence, in fine, we explain all the symptoms of the hot stage of fever. Many of the minutiae, which are not mentioned here, are also easily explained on these principles.

Thus, then, are we able to account for all that occurs in fever, not by the *vis medicatrix naturæ*, but simply by a natural property of the system.... A property as the poet has beautifully expressed it,

“ By firm, immutable, immortal laws

“ Impress’d on nature by the Great First Cause.”

IV. We shall now proceed to point out some inconsistencies, to be found in Dr. Cullen’s theory.

“ It will still, (says the author speaking of spasm) however, remain a question, what is the cause of this spasm; whether it be directly produced by the remote causes of fever, or if it be only a part of the operation of the *vis medicatrix naturæ*. I am disposed to be of the latter opinion, because in the first place, while it remains still certain that debility lays the foundation of

fever, it is not obvious in what manner the debility produces the spasm, and what seems to be its effect, the increased action of the heart and arteries; and secondly, because, in almost all cases, in which an effort is made by the *vis medicatrix naturæ*, a cold fit and a spasm are almost always the beginnings of such an effort." P. XLI and XLII.

From this, it is obvious, that the author supposes spasm to be a part of the operation of the *vis medicatrix naturæ*; but he afterwards commits himself in the following quotation: "I suppose, (says he) that in every fever there is a power applied to the body, which has a tendency to hurt and destroy it, and produces in it certain motions, which deviate from the natural state; and, at the same time, in every fever which has its full course, I suppose, that in consequence of the constitution of the animal œconomy, there are certain motions excited, which have a tendency to obviate the effects of the noxious powers, or to correct and remove them. Both these kinds of motion, are considered as constituting the disease. But the *former* is perhaps strictly the morbid state, while the *latter* is to be considered as the operation of the *vis medicatrix naturæ*, of salutary tendency, and which I shall hereafter call the *reaction* of the system.

"Upon the supposition that these two states take place in every paroxysm of fever, it will appear to be chiefly in the time of the hot stage that the reaction operates in removing the morbid state; and therefore, as this operation succeeds more or less quickly, the hot stage of paroxysms



will be shorter or longer. But, as the length of paroxysm depends chiefly upon the duration of the hot stage, so the longer duration of this and of paroxysms must be owing either to the obstinacy of resistance in the *morbid state*, or to the weakness of the *salutary reaction*, and it is probable that sometimes the one, and sometimes the other of these circumstances takes place.

“ It seems to be only by the state of the *spasm* that we can judge of the resistance of the *morbid state* of fever, and with respect to this spasm I observe, that either the causes exciting it may be different in different cases, or, though the cause should be the same in different persons, the different degree of irritability in each may give occasion to a greater or lesser degree of spasm, and therefore, the *reaction* in fever being given, the continuance of the hot stage, and of the whole paroxysm, may be longer or shorter, according to the degree of *spasm* that has been formed.”  
P. LIX, LX, and LXI.

From these quotations it is plain, that our author is guilty of an inconsistency, or contradiction in principles. In the first instance, he believes the spasm to be a part of the *curative* operation of the *vis medicatrix naturæ*, and assigns his reason for this belief; but, in the second, considers it as a part of the *morbid state*, and supposes that the obstinacy, or rigidity, with which it resists the exertion of the *vis medicatrix naturæ*, or the weakness with which this power is performed, may account for the difference between the duration of one paroxysm of fever, and another.



But again, the Dr. tells us that a spasm is induced on the extremities of the arteries during a cold fit of fever : “ It is particularly observed, that during the cold stage of fever, there seems to be a spasm induced every where on the extremities of the arteries, and more especially of those upon the surface of the body” P. XL. ; and soon afterwards observes, “ But at the same time, it seems to me probable, that during the *whole course* of the fever, there is an *atony* subsisting in the extreme vessels, and that the relaxation of the spasm requires the restoring of the tone and action of these.” P. XLIII.

Now, we really cannot conceive how the opposite states of spasm and relaxation can exist at the same time in the extreme arteries or vessels! Probably it may be said, the author means, that only the extreme ends of the extreme vessels, are affected with spasm, while the other portion of these vessels remains in an atonic or debilitated state. We doubt whether this were Cullen’s idea ; however, if it were, we shall shew it to be inconsistent, from a quotation we shall presently bring forward, with those he has advanced in other parts ; thus,....“ One cause (says he, when speaking of the obstinacy of spasm) of the obstinacy of spasm in fevers may be clearly perceived. In inflammatory diseases there is *diathesis phlogistica* prevailing in the body, and this diathesis we suppose to consist in an increased tone of the whole arterial system. When, therefore, this diathesis accompanies fever, as it sometimes does, it may be supposed to give occasion to the febrile spasm’s

being formed more strongly, and thereby to produce more protracted paroxysms. Accordingly we find, that all inflammatory fevers are of the continued kind; and that all the causes of the diathesis phlogistica have a tendency to change intermittents into continued fevers. Continued fever, then, being often attended with the diathesis phlogistica, we conclude that, in many cases, this is the cause of their continued form." P. LX.

Here, then, our author has accounted for the production of continued fevers! But let us examine this: How is it possible for the spasm to owe its rigidity to a *phlogistic diathesis*; or for an atony to subsist in the extreme vessels, during the presence of such a diathesis; or lastly, for this diathesis to occur without immediately removing both the spasm and atony? For we are told it consists "in an *increased tone* of the *whole* arterial system," and again, that "during the whole course of fever there is an atony subsisting in the *extreme* vessels, and that the *relaxation* of the *spasm* requires the restoring of the *tone* and action of these." Now, will not the occurrence of a phlogistic diathesis, or increased tone of the whole arterial system, remove the atony and spasm altogether, instead of increasing the rigidity of the latter? And where now rests the Doctor's explanation of continued fevers?

V. After writing the above, we expected to have found further and more important remarks on Cullen's theory, by the perusal of an octavo volume, entitled, "Observations on the old Sys-



tems of Physic," by Dr. Brown, whose theory we have already considered; and were really surprised at finding scarce an observation worth noticing. He misrepresents Cullen's meaning, by quoting half sentences, and intermixing with them his own remarks. He is also extremely illiberal, abusive, and vain throughout: as a proof of this, the following quotation will be sufficient: "This brat" (Cullen's theory) "the feeble, half-vital, semiproduction of phrenzy, the starveling of strained systematic dullness, the forlorn outcast of the fostering care to which it owed its insect vitality, was now to be pampered by a crude and indigestible nutriture, collected from all the materials which had composed the several fabrications of former erroneous systems, was now to be decorated with every foreign plumage; and in this, its totally borrowed and heterogeneous form, instead of the hideous caricatura, which it was, contrived to excite the derision of mankind: it was to be ostentatiously obtruded upon the world as a new and respectable doctrine, and held up, forsooth, as the formidable rival of a splendid system."

Notwithstanding this rancour, this phrenzy, this specimen of fury and malice, we think considerable apologies may be offered for its author. For it must be remembered, that Brown was once the inmate and confidential friend of Cullen; whose system of medicine he defended at all times, and against all who opposed it; and yet, after this, to be crossed, disappointed, opposed, and kept down in every exertion to rise, by the very man from



whom he should have expected support, was no small mortification, and could not fail to cool the fervour of his friendship.

Again, Brown was the author of what he, at least, conceived a *splendid system*; and which certainly stood high on the lists, for medical merit: to find this opposed and ridiculed, himself neglected and persecuted, was enough to make him thus harrow up the doctrines of his preceptor, to pour his whole soul on paper, and to write as bitter as gall. But we digress: it is not our province to extenuate the foibles of Brown: let us hide them in forgiveness, and hail him in the language of Zanga:

“ .....all thy good  
Now blazes; all thy guilt is in the grave.”

*Trag. Revenge.*

To return. We shall conclude these remarks on the Cullenien theory of fever, by observing, that although the author has been unsuccessful in his speculations here, as on many other occasions, we must not omit to pay due homage to his memory. For his accuracy in observing and describing the symptoms of disease, and for his numerous and important facts, he was not excelled even by Sydenham himself. Notwithstanding his errors in theory, we must still view Dr. Cullen as an improver in our science,....as a star of the higher order, in the firmament of medicine.

## DARWINIAN THEORY.

I. From the compressed view we are necessarily obliged to take of the Zoonomia, probably the subsequent remarks may be rendered intelligible to those only who are familiar with that work.

The section on "Stimulus and Exertion" contains those principles, which constitute the ground-work of the Darwinian theory, and to which therefore the reader is referred. An abridgment of them is briefly as follows.

1. There is diffused throughout the animal system a certain property, which Darwin has called the sensorial power, and which answers to the excitability of Dr. Brown.

2. When a stimulus is applied to the body, it acts upon it by means of this sensorial power, and produces a certain effect, which our author calls exertion, and which is only another name for excitement.

3. If the stimulus be greater than what is common, it exhausts the sensorial power; but, if it be less, this power becomes accumulated.

4. The exertion is varied; first, by the quantity of sensorial power; secondly, by the quantity of stimulus; and thirdly, by the proportion these bear to each other.

5. If both the sensorial power and stimulus are equal, and of a natural quantity, the exertion produced is moderate, and constitutes health.

6. When the exertion becomes preternaturally increased, either from an excess of stimulus,



or from an excess of sensorial power, or from an increase in the quantity of them both, it constitutes disease.

7. And when the exertion becomes much diminished, either from a deficiency of stimulus, or from a deficiency of sensorial power, or from a deficiency of them both, it also constitutes disease.

8. The former kind of disease is to be cured by *reducing* the increased exertion, by bleeding, purging, vomiting, low diet, cool air, &c.

9. The cure of the latter is to be effected by *increasing* the exertion, by stimulating remedies, as opium, spirituous liquors, nourishing diet, heat, electricity, &c.

These are the roots from which spring the luxuriant plants of the Zoonomia; or, in other words, these are the first principles upon which Dr. Darwin has founded his medical theories. From this short sketch the reader will perceive that the Darwinian and Brunonian theories strongly resemble each other; they also, however, differ widely, as will now be seen.

II. It is the common opinion that these two theories are the same, or that they differ from each other but little; and it has also been supposed that Darwin was indebted to Brown for his principles. This opinion we think unjust; although he might probably have profited something by the *Elementa Medicinæ*, which were published fifteen years before the Zoonomia: nay, we cannot admit even this, if we give any



credit to what he tells us himself, *viz.* "that a great part of this work" the *Zoonomia* "has lain by the writer above twenty years, as some of his friends can testify:" and again, "the coincidence of some parts of this work" *Zoonomia* "with correspondent deductions in the Brunonian *Elementa Medicina*, a work (with some exceptions) of great genius, must be considered as a conformation of the truth of the theory, as they were probably arrived at by different trains of reasoning." But, however it may be determined, about their claim to originality, it is certain they agree in many points, and disagree in many others of their respective theories.

1. They agree in believing that the application of a stimulus will increase the exertion or excitement, and that its abstraction will lessen it.

2. They agree in supposing that the exertion or excitement, simply varied in degree constitutes disease, and also, that it constitutes health when at a medium.

3. They also agree in supposing that a diminished action from the abstraction of stimulus, is accompanied or followed by an increase of sensorial power or excitability.

1. But they disagree with respect to the manner in which this power is affected, when the diminished action is from an excessive application of stimulus: Brown, supposing that in this case, it is always exhausted; while Darwin seems to think that it is sometimes accumulated; however he is very confused on this point as will be seen hereafter.

2. Although they both agree as to the source from whence this power is derived; Brown does not attempt to account for its manner of production; while Darwin does, and supposes it to be a secretion from the brain.

3. They disagree in their opinions concerning the nature of stimulus: Brown supposing all stimuli to be of the same nature, varying only in their degree of force; while Darwin contends that they are different in their nature, or, in other words, that they may be considered of specific kinds; some being particularly adapted to act on one part of the body, and some to act particularly on another; thus tartar emetic taken into the mouth produces no irritation, but when swallowed produces so great an irritation in the stomach as to invert its motion, and hence brings on a vomiting. Again, ipecacuanha acts on the stomach, mercury on the salivary glands, cantharides on the bladder, &c.

4. Brown supposes that an increase either of excitability or of stimulus, so as to produce an increase of excitement, will produce an increase of strength; whereas Darwin expressly says, that to produce strength, it is necessary the quantity of sensorial power and the quantity of stimulus should *both* be increased.

5. Brown divides diseases into two kinds, those of increased, and those of decreased action, which he calls *sthenic* and *asthenic*. Darwin also supposes that these different degrees of excitement constitute disease; but he also divides diseases according to their peculiar symptoms into four classes, *viz.* diseases of irritation, those of



sensation, those of volition, and those of association, all of which he again subdivides into orders, genera, &c.

6. Brown stimulates, only in diseases of diminished action, and deplets only in those where the action is increased: Darwin stimulates too in diseases of weak action, but he often both stimulates and depletes in those where the action is increased.

7. Their theories of fever are different; Brown supposes fever to consist merely in debility and to be induced, immediately by debilitating causes: Darwin divides fever into two kinds, simple and compound, and accounts for its production by a very ingenious, though a very unsatisfactory and objectionable theory.

8. They also differ about the cure of indirect debility or which is the same thing, the torpor from excessive action. Brown purposes curing it by stimulating only, beginning with a stimulus, a little less than that which brought on the debility, and gradually reducing it until the cure is completed: while Darwin sometimes stimulates and sometimes abstracts stimulus in order to let the sensorial power accumulate. He also tells us, that after the application of a strong stimulus a lesser one will produce no effect; and that if we apply a stimulus greater than that which produced the torpor, instead of curing this, we shall only exhaust the sensorial power more, and bring on a greater torpor. This of course militates strongly against Brown's method of cure and also against his own; for, according to these principles, stimulus can be of no use in any part of the cure.



III. Having thus stated the particulars in which the Darwinian theory differs from the Brunonian, as well as those in which they agree; we shall now proceed to offer a few objections to some of its principles.

One very common objection to this theory is the manner in which the production of the sensorial power is accounted for. The author, as before observed, supposes it to be a secretion from the brain, this however, is by no means proven, on the contrary there are many arguments to oppose it. But as there certainly does exist such a power in the living body, it does not matter much about the manner of its production, nor by what name it is called, whether excitability, sensorial power, vibratility, or any other name; we shall therefore pass on to the doctrines that Darwin has founded on the *laws* of this property. But previous to our doing this, we shall pause a little to notice, as connected with this subject, what we conceive to be very poor reasoning in support of a very absurd opinion: the reasoning and opinion alluded to, will be found in the Doctor's section on the impenetrability of matter.

Thus after having remarked that Galilæo, Mitchel, Boscovich, and Priestley, inclined to the belief that matter was not impenetrable, he proceeds in support of the opinion: "The uninterrupted passage of light through transparent bodies, of the electric æther through metallic and aqueous bodies, and of the magnetic effluvia through all bodies, would seem to give some probability to this opinion. Hence it appears,

that beings may exist without possessing the property of solidity, as well as they can exist without possessing the properties which excite our smell or taste, and can thence occupy space without detruing other bodies from it; but we cannot become acquainted with such beings by our sense of touch, any more than we can with odours or flavours, without the sense of smell or taste." Our author then goes on to apply this doctrine, of the penetrability of matter, to the sensorial power, or spirit of animation (for which purpose we rather suspect the Dr. introduced it); and supposes that its occasionally assuming the property of solidity, and again laying it aside, analogous to what we are taught of spirits and angels, will account for all the phenomena of animal motion.

We have but few remarks to offer on this doctrine. As to the power angels may have, of assuming the property of solidity, and of divesting themselves of it at pleasure, we know but little, and it is foreign to our subject: we are, however, certain, that the author has not proven matter to be penetrable, or that the sensorial power bears the smallest resemblance to angels. The circumstance of light passing through transparent bodies, the electric æther through metallic and aqueous ones, &c. upon which he chiefly grounds this doctrine, can be easily explained without admitting that one particle of matter can occupy a space that is at the same time occupied by another. For metallic, aqueous, and transparent bodies, as well as all others, we know, are more or less porous, and of course the matter of electricity, or light, may



pass through those pores without occupying any space, which is at the same time occupied by a particle of these bodies : “ My appeal (says Darwin), is to common sense,” so is mine ; and as the impenetrability of matter is one of those self evident truths that do not admit of demonstration, we think our appeal is made to a favourable tribunal.

We now return to examine some of the reasonings and principles of Darwin.

1. As the Darwinian and Brunonian theories agree in making disease to consist, only in different degrees of healthy excitement ; the objections before urged to that part of Brown’s, will also lie in full force against this part of Darwin’s.

2. In his fundamental section of stimulus and exertion, and when speaking of fever sometimes curing itself ; our author supposes, that one way in which this may be effected, is the following ; “ So much pain is introduced into the system by the violent contractions of the fibres, that inflammation arises, which prevents future cold fits by expending a part of the sensorial power in the extension of old vessels or the production of new ones ; and thus preventing the too great accumulation or exertion of it in other parts of the system ; or which by the great increase of stimulus excites into great action the whole glandular system, as well as the arterial, and thence a greater quantity of sensorial power is produced in the brain ; and thus, its exhaustion, in any peculiar part of the system, ceases to be effected.”

This is surely a slippery mode of reasoning: For, in the cure of fevers, our object, it appears, is to avoid the torpor or cold stage, with its concomitant, an accumulated sensorial power, as it is these which cause or produce the hot fit. Now we should suppose that the stimulus arising from the inflammation, which the author speaks of, added to the stimulus of the irritation, which is the cause of the fever would, instead of preventing torpor, have a contrary effect, and bring it on the sooner. It is true we are told that a part of the sensorial power is expended by the inflammation, either in the production of new vessels, or in the extension of old ones; and that hence, the expenditure of it is prevented in other parts of the system, in consequence of which, the torpor and succeeding exertion, which constitute fever, is prevented. But we do not at all see how such a consequence is to follow. For the irritative stimulus which induces the fever, must expend a portion of the sensorial power, and it is obvious, that the additional stimulus of inflammation must exhaust another part, no matter in what *way* it acts, nor in what *part*, whether in the extension of old vessels or in the production of new ones, whether on the sanguiferous system or on the system at large; still its action expends sensorial power and must assist in producing torpor and the consequent hot fit of fever, instead of preventing them.

As to the latter explanation of our author's, we think it as exceptionable as the first, if not more so. For if the inflammation causes an increased



secretion of the sensorial power, as he supposes; whether this is effected either by exciting the glandular system, or in any other way, the very effect is produced which should be avoided; to wit, an accumulated sensorial power, for this is soon followed by an increased exertion; and hence the fever becomes renewed, instead of being prevented.

We have adduced the above, as a specimen of our author's reasoning in many other parts of the *Zoonomia*; and it is obvious, that according to this manner of reasoning, it will not be a difficult matter to explain almost any fact, or to prove almost any position. Thus, for instance, I will contend that the sensorial power can never become exhausted, however great the stimulus may be that is applied: For when the irritative motions produced by this stimulus, become greatly increased, they excite painful sensations and a consequent inflammation, which acting on the glandular system, as well as on the arterial, of course acts on the brain, and thus produces an increased secretion of sensorial power, instead of an exhaustion of it. This, however, is incorrect, and the fallacy of the reasoning may be easily detected.

2. Again the Doctor in laying down the proper method, for the cure of increased exertion observes, that "from hence it appears, that the true means of curing *fever*, must be such as decrease the action of the system in the hot fit, and increase it in a cold fit; that is, such as prevent the too great diminution of sensorial power in the hot fit, and the too great accumulation of it in the

cold one," and afterwards goes on, "as a cold fit, or paroxysm of inactivity of some parts of the system, generally precedes the hot fit, or paroxysm of exertion, by which the sensorial power becomes accumulated, this cold paroxysm should be prevented by stimulant medicines and diet, as wine, opium, bark, warmth, cheerfulness, anger, surprise." Sect. XII. 6. 1 & 3.

Now all this is plain, easily understood, and appears to be correct; but we shall bring into view in the following quotation, another of the doctor's principles, which will excite some confusion.

"We must recollect, (says he, speaking of the sensorial power) that the sensorial power is produced in the brain and spinal marrow by fibrous actions of those glands like other secretions; and that hence an increased action of those glands by an adapted stimulus, or by association of motions, may increase the quantity of sensorial power, which increased actions of the system, may be continued by habit, after the stimulus is withdrawn. Thus, some kinds of stimuli affect particular parts of the system; as blisters affect the skin, and the stomach by association with the skin; emetics affect the stomach; cathartics the intestines; and sea-salt the perspirable glands or capillaries; but it is probable that *wine* and *opium* affect the whole system; and, when given in small repeated quantities, that they *increase* the *secretion* of *sensorial power* either by their immediate stimulus or by association; and that the strength of convalescents is recruited, as they are enabled to



digest more food, and that of a somewhat more stimulating quality." XII. 8. The Peruvian bark also, he supposes, acts in a similar manner in the cure of agues ; that is, by increasing the sensorial power in the stomach, and in parts associated with it. Certainly then we ought not to give *wine*, *bark* or *opium* to prevent the cold fit of fever; or what is more the object, an accumulated sensorial power, as directed in the first quotation ; if according to the latter, these stimuli have the effect of producing an increased secretion of this power. This objection, will apply to many other similar theories of our author's.

3. We have a remark or two more to make on this fundamental section: thus, " If the quiescence of fibres, (says the author) which had previously been subject to perpetual stimulus, continues a longer time; or their accustomed stimulus be more completely withdrawn, the accumulation of sensorial power becomes still greater, as in those exposed to cold and hunger, pain is produced, and the organ gradually dies, from the chemical changes which take place in it." Sec. XII. 2. 2.

These principles also, at first sight, appear to be correct and consistent, but we shall presently prove, according to the Doctor's own reasoning and on his own positions, advanced in other parts of the *Zoonomia* ; that a part or organ can never die from a torpor or deficient action; which will of course clash loudly with the above quotation.

First, it is a principle inculcated all through the author's work, that when a stimulus is applied

to a part, it acts in proportion to the quantity of sensorial power the part possesses, that is, if it produce a certain action or effect on a part possessing a certain portion of sensorial power, it will produce twice as great an action if the quantity of sensorial power be accumulated to twice that quantity.

And secondly, the author tells us in various other parts, that pain, pleasure, desire, aversion, and association, are all considerable stimulants ; thus, “A stimulus external to the organ, originally induces into action the sensorial faculty termed irritation; this produces the contraction of the fibres, which, if it be perceived at all, introduces pleasure or pain ; which in their active state are termed sensation; which is another sensorial faculty, and occasionally produces contraction of the fibres; this *pleasure* or *pain* is, therefore, to be considered as another *stimulus*, which may either act *alone* or in conjunction, with the former faculty of the sensorium termed irritation. Sect. XII. 2. 1.” In the same manner, he goes on to prove, that desire, aversion, and association, are all stimulants.

Now, reasoning from the preceding positions, we do not see how an organ, or part, can possibly die from a deficiency of action, possessing at the same time an increased sensorial power ; which the author has asserted to be the case. Because the pain, arising from the debility, will act as a stimulus with increased force on the highly accumulated sensorial power, produce action, dissipate the debility, and thus restore the system to its former state. Probably, it may be here said, that



as soon as an increased action is excited, the debility, and of course, the pain, connected with it must cease, and that the stimulus of pain being now withdrawn, the debility will again return. This kind of reasoning, however, is not correct, for it does not follow, that action must cease, immediately on the cessation of the pain that produced it; and the old Latin adage of *sublata causa, tollitur effectus*, does not hold universally good. For we know that a grain of sand may be washed from the eye immediately, and yet the inflammation it occasioned remain for weeks. But admitting the above argument to be correct; still nothing is gained by it, death could not be the consequence of the return of the debility; for when it returned the pain it occasions must also return, which acting as a stimulus, would produce the same routine of action as before.

4. By an extension of the above reasoning we are led to offer an observation or two on the author's second class of diseases, or diseases of sensation: and we think it will appear evident that if ever these diseases occur, they must from their nature immediately cure themselves, and of course, can never come under the province of the physician.

Diseases of sensation are said to be produced, when the irritative motions become either so excessive or so deficient, as to excite the sensorial power of sensation; and this circumstance of their being attended with sensation, is what constitutes their very essence. The sensation is, generally, of the painful kind; and hence all inflammations are arranged under this class of diseases.

Now, as pain is a strong stimulus, we should suppose, that it, when added to the stimulus of the irritative motions which produced it, together with the stimuli that produced these, would, when all combined, soon exhaust the sensorial power, bring on torpor or *insensibility*; and thus remove the disease of *sensation*. Or, if the pain arises from debility, attended with an accumulated sensorial power, which is thought to be the case in diseases of decreased sensation; then in this, as in a former case, the stimulus of pain, acting on the accumulated sensorial power, must produce action, remove the debility, and thus also cure this kind of sensitive disease.

5. The same mode of reasoning will apply to the doctor's third class, or diseases of sensation. These diseases (as the reader will see by consulting the *Zoonomia*) are supposed to consist, either in an increased, or decreased exertion of the will, and are thus produced. When the sensation of pleasure or pain becomes so great as to excite desire or aversion, volition is called into action, which being a strong stimulus, exhausts the sensorial power, producing torpor or *insensibility*; and thus relieves or removes the excessive sensation. Under this class of diseases, the author arranges convulsive diseases, diseases of the mind, &c.; explains himself elegantly, and argues ingeniously: although, as we conceive, incorrectly. For we do not see how the exertion of the will, is to exhaust the sensorial power, and thus relieve painful sensations; because this stimulus is never called in, until the sensation which produces it, has arrived



to its utmost height: now, this being the case, we see no reason why the stimulus of the pain itself should not expend the sensorial power. For the stimulus of pain, must be in proportion to the degree of pain; and, as this is excessive before volition is exerted at all, the stimulus must also be excessive before this power is exerted; and, if it be excessive, it ought to exhaust the sensorial power, and thus prevent the occurrence of volition altogether. Besides, this operation of the mind can never exhaust this power, itself; for, as it takes place only when the pain is excessive, as soon as it begins to diminish the sensorial power, and thus to lessen pain, it would, we should suppose, cease, and not be renewed until the pain became again excessive; and, upon its recurring the second time, it must produce the same routine of effects as before, and still be unable to exhaust the sensorial power; for this libration must continue as often as an excess or deficiency of pain alternate with each other; and of course, nothing is gained.

6. After having made the preceding remarks on some of the principles and reasoning of Darwin, it was our intention to have gone on to his fourth class, or diseases of association; and, as connected with this, to have entered into a full consideration of his theory of fever; but we find this would be far too extensive for our present purpose; and to notice them only superficially, would probably render our remarks unintelligible; we shall therefore pass them over. We must, however, observe, that although much ingenuity is

displayed by the author, in his theory of fever, yet the reader will also find on scrutinizing it, that there is more contradiction in principles, more sophistry in reasoning, and, on the whole, more objectionable theory here, than in any other part of the *Zoonomia*; and that the Doctor may be properly considered, throughout, as traversing the dark ocean of speculation, without rudder or compass.

Sympathy, an *expression* so much used by almost all medical writers from Hippocrates down, constitutes the *sine qua non* of this theory; and is divided by Darwin into two kinds, *viz*, *direct* and *reverse*. Direct, when a torpor in one part of the system causes a torpor in another; or where an increased action in one part causes an increased action in another. Reverse, when a torpor in one part, causes an increased action in another; or where an increased action in one part, produces a torpor in another. Now no one, it is presumed, will doubt the existence of such a property as has generally been called sympathy; but the reasoning and explanations that Darwin has founded on it, may, to borrow an expression, “mean any thing or nothing.” Thus as a specimen. Has a full meal produced a flushed face, and a glow of warmth over the whole skin? It is owing to the food, first stimulating the stomach into increased action, and secondly, the capillaries, by their *direct sympathy* with the stomach: and when the action of the capillaries becomes increased, their secretions become also increased, which are attended with an increase of heat; and this heat acts, as an addi-



tional stimulus, to the capillaries and the fine ramifications of blood vessels, that are scattered over the skin; hence we account for the flushing, and increased heat, attending a full meal.

But it sometimes happens that a full meal causes a paleness and chilliness of the skin: how is this to be explained? easily enough: in this case, the increased action of the stomach, expends so much of the general sensorial power, besides that of its own, that it is lessened in every other part of the system, and of course, in the capillaries; in consequence of which, their action becomes thus diminished, by a *reverse sympathy* with the stomach; their secretions and the heat attending these also, become diminished, while the small blood vessels of the surface, being deprived of their accustomed stimulus of heat, become debilitated and collapsed; and hence the paleness and coldness of the skin, which sometimes follows a full meal.

Again, a torpor or diminished action of the stomach will sometimes occasion an increased action of the cutaneous capillaries; and of course, redness and heat on the surface, which is thus to be explained. By a *direct sympathy*, which subsists between the heart and arteries, and stomach; when the latter becomes torpid, the former also become torpid; but, in consequence of this inactivity of the heart and arteries, they do not expend the usual quantity of sensorial power; hence it becomes accumulated in them, and as their inactivity still continues, from their association with the stomach, the sensorial power still continues to accumulate in them, until it is finally communicated to the capillaries; which, as they are not associated with

the stomach by a *direct*, but by a *reverse*, sympathy, immediately take on an increased action ; and thus produce the heat, redness, &c.

We have thus attempted to imitate Darwin's reasoning and explanations ; probably with the assistance of three or four kinds of sensorial powers ; as the sensorial power of irritation, the sensorial power of association, the sensorial power of sensation, &c. which the author always has ready ; we should have rendered our explanations more plausible. It is unnecessary to offer a comment on them, their fallacy must be easily detected. In fine, the Doctor appears here to have burlesqued theory, and *almost* rendered it ridiculous, by an excess of theorizing : like Don Quixote, who surpassed so far all other Knights-errant, in his unheard of achievements, that he entirely knocked up chivalry.

IV. We shall next proceed to bring into one view, several contradictions of a fundamental nature, contained in different parts of the Zoonomia.

1. The author, when explaining the effects and manner of operation of the cold bath, observes that, “ In those constitutions where the degree of inirritability, or of debility, is greater than natural, the coldness and paleness of the skin, with the quick and weak pulse, continue a long time after the patient leaves the bath ; and the subsequent heat approaches by unequal flushings, and he feels himself disordered for many hours. Hence the bathing in a cold spring of water, where



the heat is but  $48^{\circ}$  on Fahrenheit's thermometer, much disagrees with those of weak or inirritable habits of body ; who possess so little sensorial power, that they cannot, without injury, bear to have it *diminished* even for a short time ; but who can, nevertheless, bear the more temperate coldness of Buxton's bath, which is about  $80^{\circ}$  of heat, and which strengthens them, &c." Sect. XXXII. 3. Here then it is obvious, that the cold bath is thought to *diminish* the sensorial power ; but hear what he says in the following quotation. " Thus, on going into a very cold bath, suppose at  $33^{\circ}$  of heat, on Fahrenheit's scale, the action of subcutaneous capillaries, or glands, and of the mouths of the cutaneous absorbents is diminished, or ceases for a time. Hence, less or no blood passes these capillaries, and paleness succeeds. But soon after emerging from the bath, a more florid colour and a greater degree of heat are generated on the skin than was possessed before immersion ; for the capillary glands, after this quiescent state, occasioned by the want of stimulus, become more irritable than usual to their natural stimuli, owing to the *accumulation* of *sensorial power*, and hence a greater quantity of blood is transmitted through them, and a greater secretion of perspirable matter ; and, in consequence, a greater degree of heat succeeds." Sect. XXXII. 2. And here he tells us the cold bath *increases* the sensorial power : which of these doctrines shall we believe ? Or how can the author reconcile these contradictions ?

Again, the doctor tells us in another part, that the sensorial power is accumulated by the *abstrac-*

tion of stimulus ; and directly afterwards, that it is accumulated by the *application* of stimulus....

“ Thus, when the skin is exposed to great *cold*, the inactivity of the cutaneous capillaries is diminished, and in consequence an *accumulation* of *sensorial power* obtains in them, because they are usually excited into incessant motion by the stimulus of heat. Contrarawise, when the vessels of the skin are exposed to great *heat*, an *excess* of *sensorial power* is also produced in them, which is derived thither by the increase of stimulus above what is natural.” Class III. i. 1. Here, we think, is an absurdity ; for if the sensorial power were increased, both by the application, and by the abstraction, of stimulus, nothing could diminish it. It may be said, that the author supposes stimulus to produce an accumulation of sensorial power, *only* by inducing torpor, or indirect debility ; and this, in fact, does seem to be his meaning. But if so, it contradicts a principle before advanced, viz. that the *stimulus* of opium, wine, &c. increases the secretion of sensorial power, *without* inducing torpor. Besides, the principles of the following quotation, militate against this idea : “ This (says he, when speaking of torpor relieving pain), accounts for the relief which is received in all kinds of pain, by any violent exertion of our muscles, or organs of *sense*, which may thus in part, be ascribed to the *exhaustion* of sensorial power by such exertions.” C. III. i. 1. Now these exertions, we are told in other parts, are strong *stimuli* ; here then we see, indirect debility or torpor from an excess of action produce,



not an *accumulation* but an *exhaustion* of the sensorial power.

As this is an important point to be determined, we shall pursue the subject; to see how far Darwin's views of it are clear and connected.

We are told, in vol. 2, p. 331, that the torpor from an *abstraction* of stimulus, is followed by an *accumulation* of sensorial power; while the torpor from an *excessive* stimulus, is followed by a gradual restoration of its *natural quantity only*, but never by an *accumulation* of it: thus, "It must be observed, that those parts of the system which have been for a time quiescent from the *want* of stimulus, as the vessels of the skin when exposed to cold, acquire an *accumulation* of sensorial power during their inactivity; but this does *not* happen *at all*, or in much less quantity, from their quiescence after great expenditure of sensorial power by a previous *excessive* stimulus, as after *intoxication*. In this case, the muscles or organs of sense, gradually acquire their *natural* quantity of sensorial power, as after sleep; but not an accumulation or superabundance of it. And by frequent repetitions of exhaustion by great stimulus, these vessels cease to acquire their whole natural quantity of sensorial power, as in the scirrhus stomach, and scirrhus liver, occasioned by the great and frequent stimulus of vinous spirits; which may properly be termed irritative paralysis of those parts of the system." So far so good: but now listen to what he tells us when explaining the effects of repeated stimuli. "When a stimulus is repeated more frequently than the expenditure of

sensorial power can be renewed in the acting organ, the effect of the stimulus becomes gradually diminished. Thus if two grains of opium be swallowed by a person unused to so strong a stimulus, all the vascular systems in the body act with great energy, all the secretions and the absorption from those secreted fluids are increased in quantity; and pleasure or pain is introduced into the system, which adds an additional stimulus to that already too great. After some hours the sensorial power becomes diminished in quantity, expended by the great activity of the system; and thence, when the stimulus of opium is withdrawn, the fibres will not obey their usual degree of natural stimulus, and a consequent torpor or quiescence succeeds, as is experienced by drunkards, who on the day after a great excess of spirituous potation, feel indigestion, head-ache, and general debility.

“ In this fit of torpor or quiescence of a part, or of the whole of the system, an *accumulation* of the sensorial power in the affected fibres is formed, and occasions a second paroxysm of exertion, by the application *only* of the *natural* stimulus; and thus a *libration* of sensorial exertion between *one excess* and *the other*, continues for two or three days, where the stimulus was violent in degree; and for weeks, in some fevers, from the stimulus of contagious matter.” Sec. XII. 3. 1.

And here, the author not only supposes the indirect debility or torpor, from an excess of action, to be accompanied with an accumulation of sensorial power; but makes use of the fact to explain the frequent recurrence of the paroxysms of some fevers.



We have thus given a few examples, wherein our author has advanced direct contradictions in fundamental principles; *others* might be adduced, but the limits of this essay must necessarily prevent our stating them.

V. Having thus far occupied our remarks on the Darwinian theory, in pointing out, what we conceived, erroneous reasoning, and contradiction in principles; we shall finish them, by noticing two or three of its beautiful and ingenious speculations; among which are the doctrines of instinct, retrograde motion of the absorbents, &c....how far they are *correct*, we shall not determine.

1. Of instinct. Darwin rejects entirely the ideas generally attached to this term, and proposes to account for all the phænomena that have been referred to this power, by the laws of physical necessity: that is, instead of a kind of '*inspiration*, or a *divine something*,' prompting the young of an animal to the performance of certain motions; as sucking, walking, &c. Our author supposes, that it is led to perform those motions simply by the properties of its *natural* or *physical organization*. He supposes, that the fœtus in the earliest stages of its existence, possesses irritability; and, that from this property, are derived originally and gradually, all the senses, associated motions, and ideas; and that from the laws of these arise all those actions, which have generally been referred to a preternatural power, called instinct.

Thus, when the mouth of a new born infant is applied to the mother's breast, the warmth of the

breast giving a pleasurable sensation to its cheek, and the odour of the milk delighting its sense of smell prompt it to suck : the milk is palatable to its sense of taste, and it is prompted to suck again by an association of agreeable ideas ; and finally to repeat it, by the efforts of volition. The act of deglutition was learnt previous to birth, by swallowing a part of the liquor amnii, in which it floated ; and, probably, the muscles used in suction, as the buccinators sphincter oris, &c., are first excited into action by the warmth of the breast. Upon the same principles, the author accounts for the origin of all our sensual or intellectual motions, as our ideas of beauty, deformity, &c. For instance, the ideas of beauty are supposed to originate, not from any thing inherent in the object admired, but from particular circumstances connected with it. Thus, a sight of the female bosom gives, universally, the idea of beauty, in consequence of the pleasurable sensation of its warmth, the agreeable odour and palatable taste of its milk, &c., being originally associated with it, in the mind of the infant : and this association is afterwards extended to other objects ; hence the preference given by children, to bodies of an oval form, as marbles, balls, &c. &c. And hence, also, the rapture, with which we gaze on the distant hills of a landscape, while we behold with indifference the extensive plain that surrounds it : and hence, too, “we find, according to the ingenious idea of Hogarth, that the waving lines of beauty were originally taken from the temple of Venus.”

For much entertainment, useful information, and ingenious speculation, we refer the reader to



this part of the *Zoonomia*. He will also find, that in this section the author attacks, and completely refutes, the ridiculous idea, that of all the animated creation, man alone possessed the power of reasoning.

2. Retrograde motion of the absorbents. The opinion, that the motion of the absorbents becomes sometimes inverted or retrograde, is, at least, entitled to the claim of ingenuity. This doctrine first made its appearance in the form of an Inaugural Dissertation, by Charles Darwin; but, from several circumstances, it is highly probable, that the old Doctor, his father, was the real author. It would be too lengthy to give even a sketch of this doctrine; we mention it only, as one among the many ingenious ideas of Darwin. There are certainly some facts that cannot be well explained on any other principles. Thus, the circumstance of a cold sweat around the neck and temples, immediately relieving the difficulty of breathing in an anasarca of the lungs, admits an explanation from this doctrine, only. The translation of matter from one part of the body, to another, the phenomena that occur in the aqueous, chyliferous, and mucilaginous species of diabetes; together with many other facts contained in the *Zoonomia*, render this doctrine very probable.

3. Darwin's theory of secretion, may also be noticed; although it is by no means free from objection. He supposes that each gland possesses a peculiar appetency, by which it selects its appropriate substance, or fluid, from the general circulating mass; and here, the doctor appears to refer the whole

phenomenon to the *action* of the glands : but from some observations in his second volume it would seem, that he thought the process to be also in part a *chemical* one : “ These (says he, speaking of the increased action of the glandular system), are always attended with an increase of partial, or of general heat ; for the secreted fluids are *not simply separated* from the blood, but are new *combinations*, as they did previously exist as such in the blood-vessels. But all new combinations give out heat *chemically*, hence the origin of animal heat, which is always in proportion to the secretions of the part affected, or to the general quantity of the secretions.” Our author’s speculations here, are certainly more plausible, than those commonly met with on the subject of secretion ; but as this is, and will probably remain for ages yet, one of the most hidden secrets of physiology, we shall not notice further the doctor’s opinions concerning it.

4. Lastly, our author’s speculations on the subjects of generation, and ocular spectra, together with those that may properly be called metaphysical, including the nature and operations of the mind, are learned, ingenious, and highly entertaining.

In fine, upon a review of the whole Darwinian theory, as well its errors, as its beauties, we feel disposed to applaud, even where we see it evidently fallacious ; “ we become (to use the language of a celebrated author, on another occasion), like the artist who, after having admired a beautiful statue, used his efforts to persuade himself that it respired, and removed every thing that could dissipate his illu-



sion. We take up his work with the pleasure resembling that of the man who turns again to sleep, in hopes of prolonging the deception of an agreeable dream."

But though Darwin stands thus high as a philosopher, he stands no less conspicuous as a poet. If we view with admiration the boldness of his genius, in penetrating and unravelling the intricacies of science; we listen with delight when his muse shakes her tuneful wings in the regions of fancy;....nay more,

"Seraphic sounds enchant this nether sphere,

"And list'ning *angels*, *lean* from heav'n to hear."

*Temple of Nature.*

But to conclude this part of our essay. As we have already seen those points on which the Darwinian and Brunonian theories agree, as well as those on which they disagree; we shall finish the subject by drawing, in a few words, a comparison between them.

Darwin argues ingeniously, extends his principles far, and by turning and twisting them, often makes them explain even opposite facts; and when detected and routed in one quarter, rallies in another, and again defends himself. Brown argues plainly, but with force; never contradicts one explanation by another; and when once refuted, has no subterfuge left. The Darwinian theory may be considered as a beautiful and spreading willow, which bends unhurt before the storm; while Brown's, like the venerable and sturdy oak, withstands its ground, refusing to give way; but when once forced....falls to rise no more.

## Dr. RUSH's THEORY

Comes next under our consideration. We propose to review the doctor's theory of fever only, because he applies this, with but little variation, to all other diseases, or rather, to all other *forms* of disease.

I. A summary of this theory, is briefly as follows. According to the author, there are four causes concerned in the production of fever: first, the remote; secondly, the predisposing; thirdly, the exciting; and, fourthly, the proximate.

1. The remote causes. These are certain agents which when applied to the system, produce debility.

2. The predisposing cause. This consists in debility, which may be of two kinds: *viz.* direct, and indirect: *direct*, when from a deficiency of stimulus: *indirect*, when from an excess of stimulus. Both of these, if suddenly induced, are accompanied with an accumulation of excitability; but, if of a chronic nature, they are both attended with an exhaustion of this property. It is only under the first of these circumstances that either cannot act as a predisposing cause. The terms, debility from *abstraction* and debility from *action*, have lately been substituted by the author, for those of direct and indirect debility; in consequence of the tendency which the use of these terms have had to associate his principles with those of Dr. Brown's.

3. The exciting causes. There are certain irritants or stimulants, which acting on the accumulat-



ed excitability, produce irregular or convulsive action in the blood vessels.

4. The proximate cause, is the disease or fever itself: and consists of the above irregular, convulsive, or, as the author terms it, *morbid action* of the blood vessels.

Arguments are adduced to substantiate each of the above positions; but the author dwells particularly on the opinion, that the morbid action is of an *irregular* or *convulsive* kind; and, the more to confirm this, draws an analogy between the action of the blood vessels in fever, and the action of the nervous system in convulsive diseases; which proves to be close in a number of instances.

II. From this view of the general leading principles of Dr. Rush's theory, the difference between it and Dr. Brown's, must appear obvious. However, this point has been disputed; and therefore, in order to render the dissimilarity the more striking, we shall quote the author's own words. "From the view I have given of the state of the blood vessels in fever, the reader will perceive the difference between my opinion and Dr. Brown's on this subject. The doctor supposes fever to consist in debility.... I do not admit debility to be a disease, but place it wholly in morbid excitement, invited and fixed by previous debility. He makes a fever to consist in a change only of the natural action of the blood vessels....I maintain that it consists in a preternatural and convulsive action of the blood vessels. Lastly, Dr. Brown supposes excitability and excitement to be equal in fever....My theory supposes fever to be

the reverse of this. It consists in unequal or divided excitement and excitability. Health consists in the equality and uniformity of them both; and the business of medicine, as I shall say hereafter, is to equalize them in the cure of fever; that is, to abstract their excess from the blood vessels, and restore them to other parts of the body." *Inq. and Observations*, vol. 4.

Although from this quotation, the difference between the two theories, is plainly delineated; yet there appears to be a misstatement of the Brunonian theory, which we presume, was by no means, intended. "Dr. Brown (says our author) supposes excitability and excitement to be *equal* in fever." This is certainly not his theory; for Brown we know was of the opinion, that fever is a disease of direct debility, and of course, that the excitement and excitability are *unequal*, to wit, the excitement diminished, and the excitability accumulated. Dr. Brown, though, also supposed, that the excitement and excitability are *nearly* equally affected in every part of the system, during a paroxysm of fever; that is, that when the excitement is diminished in the blood vessels, it is also diminished, more or less, in every other part. It is this opinion, we presume, that Dr. Rush alludes to; and we presume so, because his own theory entirely rejects this idea of Brown's: and supposes, that the excitement may be, and is in fever, preternaturally increased in the blood vessels, while it is preternaturally diminished in the muscles.



III. Having thus stated, as briefly as we could, Dr. Rush's theory of fever, the principles of which, as before observed, he applies to all other forms of disease ; and shown, in his own words, wherein it differs from Dr. Brown's : we shall proceed to notice some objections that have been brought against it.

1. One objection which has been much urged to this theory, is, that it confounds the proximate cause with the fever or disease itself.

This we admit is the case : but it is an error in language only, not in ideas, and of course, an objection of this kind will carry with it but little weight. Besides, the author speaks of the proximate cause as the disease itself, only in uniformity to custom ; and uses the expression, as he tells us himself, in common with Cullen, Boerhaave, Gaubius, and others. To say that a cause and its effect can be the same, would certainly be very poor logic ; but this theory advances no such a position. For, as before observed, it supposes four causes concerned in the production of fever, *viz.* the remote, predisposing, exciting, and proximate : the remote causes, produce debility, which is the predisposing cause ; and the exciting causes, acting on this, produce the proximate, consisting in an irregular convulsive or morbid action ; which, in correct language, should be called the fever itself ; but which might be called by any other name, provided it conveyed correct ideas of its *peculiar* nature.

Although we do not contend for incorrect language, in works of science, but, on the contrary,

think it should be rejected as soon as possible ; yet since, in this instance, the ideas it conveys are clear and connected, we hope this objection will cease to be brought forward ; and that, instead of reasoning from *words*, we may learn to reason from *ideas*.

2. It has also been objected to this theory, that it supposes fever, and other forms of disease, to consist in morbid action ; while the author, in his lectures, tells us, that in synchophy and asphixia, there is an absence of all motion.

In replying to this it must be observed, that synchophy is frequently the effect of mere debility, and takes place when no morbid action is present ; or, it may also occur after violent morbid action, in which case it is owing to a suffocation of morbid excitement, and may be considered as a kind of second indirect debility where little or no action is present ; although probably in no instance does motion cease entirely.

3. A third objection that has been advanced is, that, “ according to this theory, fever has two predisposing causes, debility and a disposition to preternatural motion in animal matter ; or a connexion between excitability and debility.” The urger of this objection was certainly not acquainted with the theory ; or he wished to misrepresent it. For it by no means inculcates that debility is one predisposing cause, and a disposition to preternatural motion in animal matter another. It will appear clear to the reader of the smallest penetration, that this disposition to preternatural motion, is only another name for an accumulated excitability ; and he will also perceive, that



debility can never act as a predisposing cause of fever, or of any other form of disease, unless accompanied with this "*disposition*;" so that, instead of each constituting a predisposing cause, they must be *combined* to constitute the one.

IV. Having thus endeavoured to answer some objections that *have been* opposed to Dr. Rush's theory, we shall now mention one or two others that *may be* proposed. In doing this, we shall not pause to make any apology to its author; for we are too well acquainted with his liberality, (witness the freedom with which he relinquished his long-taught doctrine of digestion) and are too well convinced of his love of truth, to suppose our doing so will give the least offence.

1. This theory asserts, that "there is but one remote cause of fever, and that is *stimulus*. p. 132. But, it asserts in other parts, that cold, excessive evacuations, fear, &c. all of which, we know, are directly *debilitating*, instead of being *stimulants*, and frequently induce the debility of predisposition: of course, they must be considered as *remote* causes.\*

2. Again, a morbid action seated in the blood vessels, particularly in the arteries, is supposed to constitute the proximate cause of fever; and this morbid action is defined to be a convulsive one.... "I go on (says the Doctor) to remark that a fever,

\* The Doctor in his Lectures has corrected the word "remote," and instead of it, used "existing," in speaking of the unity of stimulus.

when not misplaced, consists in morbid excitement, and irregular action in the blood vessels, more especially in the arteries:" and again "this irregular action, is in other words, a *convulsion* in the sanguiferous, but more obviously in the *arterial* system." p. 134. This, as before observed, the doctor endeavours to support by an analogy between the febrile action of the sanguiferous system, and the convulsive action of the nervous system.

But this is analogical reasoning, and can amount to no more than probability; independent, however, of this, the single circumstance of the arteries *naturally* propelling the blood by a *convulsive* action, must strongly oppose the idea that their *morbid action* when it occurs, also consists of a *convulsive one*.

Although the above objection must militate against the opinion, that a *convulsive action* constitutes the *morbid one* in fevers, it does not at all lessen the utility of the term morbid action; for by it we mean only diseased action, and it is of not much consequence in what this particularly consists, provided we know the causes that produce it, can distinguish it when present, and are able to remove it.

V. Upon the whole, Dr. Rush's theory we consider as possessing many advantages. Common observations compose the materials, and simplicity only is studied in its construction. It breaks down those artificial barriers which have subdivided disease, and draws the mind of the student from the complexity of nosology, to view with less confusion the simplicity of morbid excitement;....an expression which



contains an abridgment of nosology ;....an affection which by simply varying in its seat, or degree of force, exhibits specimens of all the classes, orders, genera, and species.

Such then, is a short sketch of the theory of fever, of my illustrious preceptor ; my account of it is taken from the fourth volume of his Medical Inquiries and Observations. As that learned and elegant work is about to undergo a new edition, I have no doubt it will appear, when retouched by the pen of its able author, in a form that will render it free from the real or supposed objections that have been brought against it. In the meanwhile we have *not the least* hesitation in saying, that it explains the phenomena of fever *much* better, and leads to a *more* successful mode of practice, than the spasmodic theory of the learned Cullen, the debilitated theory of the great, though unfortunate Brown, or than the sympathetic theory of the ingenious Darwin.

In thus taking my final adieu of you, ILLUSTRIOUS PROFESSORS, suffer me to add, that I am not solicitous about the fate of my Essay ; for conscious of its imperfections it is with pleasure I reflect, that it will soon be hidden by that mantle which in time covers even works of merit,....not only scientific merit, but merit of every kind. Thus the poet :

“ Deep-whelm’d beneath, in vast sepulchral caves,  
 “ OBLIVION dwells, amid unlabell’d graves ;  
 “ The storied tomb, the laurell’d bust o’erturns,  
 “ And shakes their ashes from the mould’ring urns.”

*Temple of Nature.*

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